Dear Alison Delhasse and co-authors,

I want to truly thank you and your co-authors for the sincere and thorough answers to many of the review comments.

Both reviewer acknowledge the significance of your study as it provides new insight on ice-climate interaction. They both appreciate your discussion on the barrier winds as an explanation for differences in centennial ice evolution.

Concerning your point-by-point answers, I appreciate that you plan to extend the description of the ice-flow model and the offline correction, which will make your manuscript more easy to follow. Moreover, you put a valuable effort into scrutinising differences in lapse-rates between the ice-sheet interior and the margin. However, you stay evasive w.r.t. a more comprehensive analysis of changes in the overall wind pattern. Moreover, I am not sure if my initial comment (access review) on the consequences of the initialisation technique (5x period 1961-1990) is answered. Please reconsider.

On the basis of your point-by-point answers, I invite you to submit a revised manuscript, which will certainly be an improvement. For an objective evaluation of your answers and the implemented changes, I suggest that your revised article enters another review round.

Best,

Johannes Fürst

We would like to thank the editor for his diligent monitoring and constructive comments. Regarding his last comments, we intend to address two of them within this document. Subsequent to providing these responses, we have included a completed version of the point-by-point answers to both reviews with the corresponding modifications in the text.

1/ However, you stay evasive w.r.t. a more comprehensive analysis of changes in the overall wind pattern.

With the objective to be clearer in our manuscript about the general comprehension of the changes in local wind patterns, we improve our paragraph and add, as asked by the first reviewer, general wind speed and west-east wind component analyses (similar figure to Fig. 9 in the original manuscript). Modifications are as follows:

*Version **corrected** following comments of Reviewer#2:*
“The mitigation of the melt-elevation feedback in the MAR-coupled simulation is explained by the modification of the local atmospheric circulation on the margins around the GrIS. The evolution of the topography in the coupled simulation (for instance, Fig. 9e) causes a decrease in the melt increase with the elevation lowering. **The production of meltwater is the result of a positive energy balance at the surface. More**

specifically, it is the sensible heat flux (SHF) that interests us here, as it is directly proportional to surface temperature and wind speed. And it seems that these two parameters are influenced by changes in surface topography. Thus we compare the vertical temperature and wind speed patterns above both simulation topographies along a transect crossing the ice sheet. The example illustrated in Fig. 9 highlights that the north-south wind component (v-wind, positive northward) is larger in the uncoupled simulation on the grid cell at the margin of the ice sheet (inside the ice sheet, Fig. 9c and d). Secondly, the mean near-surface temperature that appears on the 2200-topography (coupled MAR) on the same grid-cell of the ice sheet margin is lower than the temperature computed on the uncoupled MAR topography while at lower altitude (Fig. 9a and b). Mitigation of the wind and the temperature in the MAR-coupled simulation explains the weaker melt increase with elevation lowering. It cannot be directly illustrated because the differences between coupled and uncoupled MAR melt on the MAR grid remain dominated by the melt-elevation feedback influence.

Temperature changes and (v)-wind speed decrease in coupled MAR simulation compared to uncoupled one can be explained by mitigation of the barrier wind impact on the surface melt. Barrier winds occur when air masses from the tundra cross the ice sheet, which acts as an orographic barrier (Van den Broeke and Gallée, 1996). These winds induce warm air advection from the tundra (warmer than the surrounding air of the ice sheet) locked by the orographic barrier and leading to northward wind (on the west coast) along the ice sheet margin. They are associated with high melt events due to increased wind speed and positive temperature along the ice sheet margins. Considering the changes in topography in MAPI-2w compared to MAPI-1w, the orographic barrier is weakened by the thinning and retreat of the ice sheet (Fig. 9e), as well as the associated barrier winds. This explains, on the one hand, why we observe a decrease in the v-wind component (north-south) in the coupled experiment. And on the other hand, it explains differences in temperature between both experiments because the MAPI-2w ice sheet experiences less warm air advection associated to barrier wind. Finally, this impacts the local gradient of melt/temperature with surface elevation. "

Becomes:

"The mitigation of the melt-elevation feedback in the MAR-coupled simulation is explained by the modification of **the local wind pattern and temperature near** the margins around the GrIS. The evolution of the topography in the coupled simulation (for instance, Fig. 11e) causes a decrease in the melt increase with the elevation lowering. The production of meltwater is the result of a positive energy balance at the surface. More specifically, **changes in sensible heat flux (SHF) account for this which is directly proportional to surface temperature and wind speed. We investigate here differences in these two parameters between the two experiments. They are both directly influenced by changes in surface topography between MAPI-2w and -1w (Fig. 11b and d). The near-surface temperature, as well as the north-south wind component, are altered along the margin, specifically the west part of the GrIS in the fully-coupled simulation (general wind speed, as well as west-east wind component differences, are presented in the Supplement, Fig. S7). To better**

illustrate that, we compare the vertical temperature and wind speed patterns above both simulation topographies along a transect crossing the ice sheet. The example illustrated in Fig. 11 highlights that the north-south wind component (v-wind, positive northward) is larger in the uncoupled simulation on the grid cell on the ice sheet margins (inside the ice sheet, Fig. 11c and d). Secondly, the mean near-surface temperature that appears on the 2200-topography (coupled MAR) on the same grid cell of the ice sheet margin is lower than the temperature computed on the uncoupled MAR topography while at a lower altitude (Fig. 11a and b). **Changes** in wind and the temperature in the MAR-coupled simulation explain the weaker melt increase with elevation lowering.

The temperature changes and (v)-wind speed decrease in coupled MAR simulation compared to uncoupled one could be explained **by local modifications of the barrier wind that mitigate the surface melt.** Barrier winds occur when air masses from the tundra cross the ice sheet, which acts as an orographic barrier (Van den Broeke and Gallée, 1996). These winds induce warm air advection from the tundra (warmer than the surrounding air of the ice sheet) locked by the orographic barrier and leading to northward wind (on the west coast) along the ice sheet margin. **They lead to** high melt events due to increased wind speed and **higher** temperatures along the ice sheet margins. **As the orographic barrier is weakened by the thinning and retreat of the ice sheet (Fig. 11e) in MAPI-2w compared to MAPI-1w, the barrier winds could be reduced as suggested by the decrease in the v-wind component (south-north) in the coupled experiment. This further would create differences in temperature between the two experiments as less warm air advection can occur due to the weaker barrier wind in MAPI-2w. This would have an impact on the local gradient of melt/temperature with surface elevation and would lead to a mitigation of melt in MAPI-2w compared to MAPI-1w where the barrier winds remain unaffected as the topography does not change.**"

2/ Moreover, I am not sure if my initial comment (access review) on the consequences of the initialisation technique (5x period 1961-1990) is answered. Please reconsider.

We complete our original answer to the question :

Editor comment: "*If I understand the experimental setup correctly, you introduce an 'initialisation of the coupling' to synchronise PISM and MAR in the period 1961-1990 (P6L3-12). Yet the strategy you suggest implies that the ice-sheet experiences very similar conditions in this 30yr-period for 5 times, meaning an evolution of altogether 150 years. Is that right? If not, ignore the rest. Otherwise, I think that PISM has already seen MAR climate before during the paleo spin-up. Admittedly, MAR has seen the observed ice-sheet geometry, yet with an elevation correction. I understand that you have to move onwards to the modelled geometry at some point in time. In 1991, you furthermore jump from ERA5 to CMIP6 climatic forcing. In this way, you introduce an SMB jump in 1961 and 1991. Moreover, you impose 150 years of additional ice-sheet evolution. I wonder if both is necessary or if you can simply switch both between observed and modelled geometries as well as from ERA to CMIP6 forcing at the same moment in time. If you fear a too large SMB bias for the latter, you could use an anomaly mode in the CMIP6 forcing. I do not see this point too* *critical as you do not aim for best sea-level projections in this study. Your main conclusions here will not be much affected. Anyway, these choices need dedicated discussion.*”

Thanks for your comment, our section is probably unclear as it stands. We use the 30-year climatology as a stable state assumption and ran actually 5 glacial paleo spinups (5 x 125 k years). Thus, we assume that the GrIS was stable around 1961-1990 and that the GrIS had a similar state 125 ka years ago. So we do not apply 5 x 1961-1990 MAR climatology to PISM, but we apply 5 times paleo conditions to PISM through a complete initialisation of the model thereby using the adapted 1961-1990 MAR climatology as a baseline and adding the temperature anomalies of the paleo time scale, 125 ka. It is true that we do not change the SMB over the paleo spinup but only change surface temperatures. These temperature anomalies propagate through the ice sheet during one glacial cycle modulating the ice rheology, its ability to deform and therefore the ice flow. the SMB field and the change of dynamics determine the new present-day ice sheet, with a new topography. This topography differs from our initial one and therefore would lead to a different SMB field for the same CESM climatology in MAR. Therefore, we run the paleo spin up again. Each time we corrected the stable state topography and climatology with MAR and used the new topography in PISM. We can add that we do not jump from ERA5 to CESM2 in 1991 for MAR, we always run MAR with CESM2 as large-scale forcing, with the historical run until 2014, and ssp5-8.5 from 2014 to 2100.

We first would like to thank the Reviewer#1 for the thoughtful comments which will help to improve our manuscript.

Major comments

1. The algorithm used to correct for elevation differences (the offline correction) is a key part of the methodology, and it is extensively mentioned in the results and discussion. I would recommend to provide a more detailed description of this correction in the methods section, and possibly mention how this algorithm was optimised for the Greenland ice sheet. This would help the reader to better understand the main conclusion of this manuscript. For instance, it is not clear now if the surface temperature was corrected with a lapse rate, and if so, using which value ?

It seems that our current description of the offline correction it's not clear and deep enough as the two reviewers pointed out. To address this issue, we propose to add a scheme to illustrate better how this correction is set up, as suggested by Reviewer#2. Furthermore, we propose to revise the explanation as follows:

P5. L. 3-10: "Before any data exchange between the models, data has to be interpolated on the destination grid because the two models were run at two different spatial resolutions (25 vs 4.5 km). The surface elevation simulated by PISM is then interpolated using a four-nearest-neighbour distance-weighted method on the MAR grid at 25 km. For the MAR variables, they are interpolated using the same method on the PISM grid at 4.5 km. However, they are further corrected by considering the difference in altitude between the two grids at the time of interpolation thanks to local vertical SMB/ST gradients. This method is described in (Franco et al., 2012) and is called offline correction hereafter. This method corrects the altitude-dependent variables (such as SMB and ST) by applying a local linear gradient of the variable according to the surface elevation differences between the current MAR grid cell, and the surrounding MAR grid cells (9 grid cells considered here to compute the vertical gradient)."

Become:

"Before any data exchange between the models, data has to be interpolated on the destination grid because the two models were run at two different spatial resolutions (25 vs 4.5 km). The surface elevation simulated by PISM is then interpolated using a four-nearest-neighbour distance-weighted method on the MAR grid at 25 km. The MAR variables are interpolated using the same method on the PISM grid at 4.5 km. However, they are further corrected by considering the difference in altitude between the two grids at the time of interpolation thanks to local vertical SMB/ST gradients. This method is described in (Franco et al., 2012) and is called offline correction hereafter. Firstly, a linear and elevation-dependent gradient (Figure R1) is calculated over the MAR grid by considering the values of the considered variable (SMB at 4.5 km in our example, Figure 1) of the eight surrounding grid cells of the current one. This gradient is specific to each PISM grid cell and is locally determined. An example of this gradient can be found in Figure RS1 in The Supplement. Subsequently, These gradients are utilised to correct the variable when it is interpolated onto the PISM grid. The correction is performed by multiplying the interpolated variable by the difference in surface elevation between the grid cells in MAR and in PISM. This offline correction is specifically employed to correct variables that are influenced by temperature lapse rate with altitude, namely temperature and derived variables.”

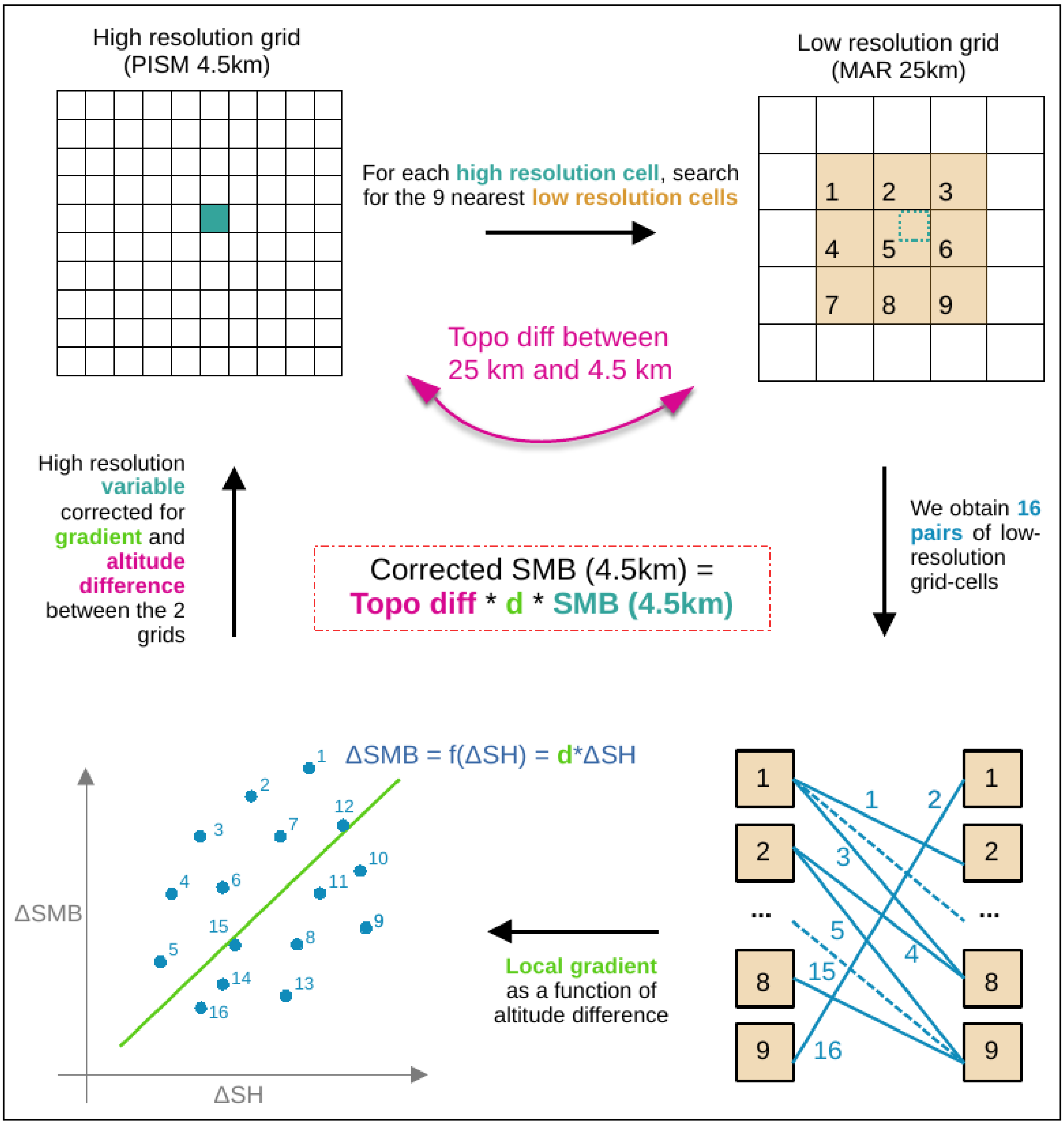

Figure R1. Steps of the offline correction as described in Franco et al. (2013). After interpolation of a variable (SMB, surface mass balance, in this figure) from a low to higher resolution grid, this variable is corrected to consider the influence of the temperature lapse rate with altitude. The correction is based on a local gradient (d) calculated by considering SMB differences (ΔSMB) between 9 nearest grid cells in the neighbourhood of the current one in the source grid in function of the surface elevation difference (ΔSH). Modified from Wyard (2015).

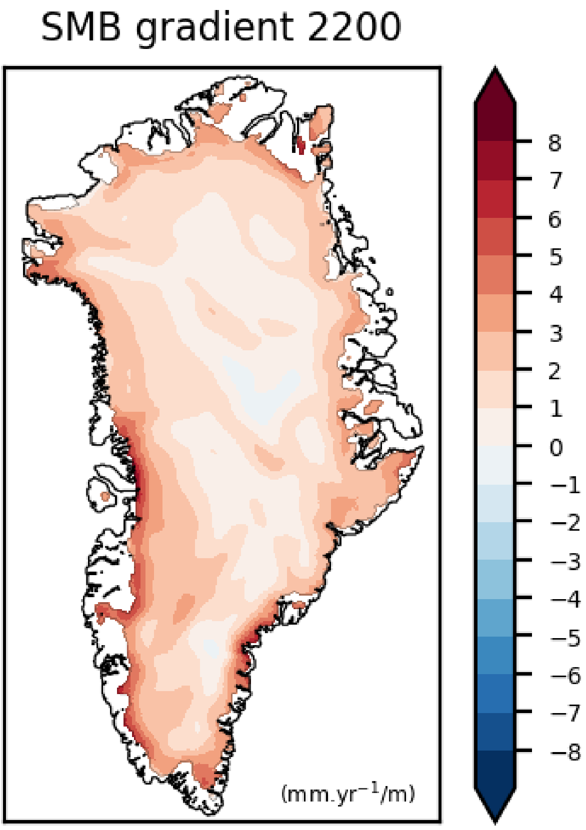

Figure RS1. Surface mass balance (SMB) gradients used to correct SMB as modelled by MAR (25 km) when interpolated on PISM grid (4.5 km) in 2200 by the MAPI-1w run (MAR-PISM uncoupled). Gradients (mm.yr-1/m) are multiplied by the difference in surface elevation to correct the rough SMB.

2. 25km horizontal resolution seems rather coarse to represent the narrow ablation zone in the most part of the Greenland ice sheet. The observed SMB can vary by a factor 2 within such a distance (e.g. on the K-transect, Van de Wal et al 2012), or even contain the entire ablation zone (e.g. on the Q-transect, Hermann et al 2018). While I acknowledge that the aim of this study is not to accurately model the SMB, it is likely that such a relatively coarse resolution strongly deteriorates the modelled wind and temperature patterns near the edges, therefore significantly changing the turbulent heat fluxes, and therefore surface melt. I would recommend to mention this in the discussion, and possibly refer to some studies which have shown that RCMs are still not yet accurately modelling turbulent heat fluxes near the edges of the ice sheet (e.g Fausto et al. 2016), or are sensitive the horizontal resolution (e.g. Franco 2012, van de Berg et al. 2020).

Thanks for your comment, it's an excellent remark that we will add to our discussion, as suggested. The reason why such "coarse spatial resolution" (25km) is used is that it's a compromise to well represent the SMB for all the ice sheet, and the MAR-computation time. Especially in a coupled mode, MAR computation time is very large. To optimize experiment time, we then used the optimal 25 km of horizontal resolution, knowing that SMB over all the ice sheet is not significantly improved by using a finer resolution.

We suggest to add these explanations at the end of the discussion:

"Another significant aspect of our method under discussion relates to the spatial resolution of the MAR model. To reach a balance between computational efficiency and adequately representing the SMB within the ensemble, we opted for a relatively coarse spatial resolution of 25 km. At the scale of the entire GrIS, this resolution proves to be a viable choice for capturing the global SMB evolution, as supported by previous research (Fettweis et al., 2020). However, at a finer scale of analysis, this resolution may compromise the accurate representation of local wind and temperature patterns. In some cases, a grid point might span an area as large as the ablation zone, introducing potential inaccuracies (Van de Wal et al., 2012; Hermann et al., 2018). RCMs remain sensitive to horizontal resolution, particularly when aiming to accurately represent SMB and Surface Energy Balance (SEB) in specific areas. The local representation of processes and surface topography by the model plays a crucial role in this sensitivity (Franco et al., 2012; van de Berg et al., 2020)."

3. One of the main results of the research is found in p12 L12 "ME depends on the sensible heat flux (SHF) related to air temperature and wind speed which are also overestimated on the margins by MAPI-1w (Fig. 7d and e)". I would recommend mentioning this very interesting result in both the abstract and the conclusion, since this is a key mechanism explaining the lower lapse rates in the coupled experiment.

As recommended, we will add this conclusion in both the abstract and conclusion. As discussed in the next comment, this could be followed by the assumption of the wind barrier that explains these differences in fluxes.

4. In the discussion, a very interesting link is suggested between the lower melt rates in the 2W coupled experiment and the mitigation of barrier winds due to drastic surface lowering. While this is a plausible explanation for the changes, this would require further analysis to properly quantify. For instance the increase in surface slope is also expected to affect the katabatic forcing in the momentum budget. Therefore I would recommend to either perform a more detailed analysis of modelled wind patterns, e.g. by investigating the entire momentum budget (van Angelen et al 2016) between the 1W and 2W experiments, or to mention in the text that changes in barrier winds are just one (of the possibly many) possible effects of surface lowering. In the conclusion (p18 L5) the reduction in barrier winds is now stated as a fact yet it has not been demonstrated in this study.

We agree that to confirm this assumption (changes in barrier wind responsible for changes in melt rate) it should be better to realise a complete wind budget. If it should be really relevant, it is also a bite outside the main goal of the paper, which is to present the coupling between MAR and PISM and an explanation of why both methods of representing the melt-elevation feedback give different results. As this comparison highlighted a new feedback link with the wind regime and which mitigates the melt-elevation feedback, it was important to try to explain it. A wind budget requires also more data than we have, meaning that we need to run again the model to obtain more detailed output concerning wind components, and at different levels. Given all these parameters, we prefer to keep this explanation with barrier wind as the main assumption to explain why we have less melt at the margins in the fully-coupled MAR version. A wind budget should be an excellent exercise to propose as a perspective of this work and will be of course explained in the discussion.

We could also add that the horizontal resolution, as discussed in the former comment is probably not the best one to realize this exercise. A finer resolution should more suitable to represent this kind of phenomenon, as well as flux budget.

The discussion, conclusion and abstract will be adapted following this comment and the answer.

Following comments 3) and 4), we suggest correcting the conclusion as follows:

"The offline correction is no longer valid on the ice sheet margins because it does not consider mitigation of temperature increase and thus of melt due to changes in topography such as retreat and changing slope. These changes affect the wind regime in the margins of the MAR-coupled simulation. Barrier winds usually act along the ice sheet, bringing warmer air from the tundra, enhancing the surface melt and increasing the northward wind speed along the west side, for example. The key element for forming this kind of wind is the orographic barrier which is reduced with the evolving topography in MAPI-2w. Consequently, the enhanced melt is mitigated as well as melt-elevation feedback. Nevertheless, it would be necessary to analyse further the current recurrence of these barrier wind events, their characteristics and the synoptic situations favourable to their development to better identify them in the projections and to analyse their sensitivity to changes in surface topography. We concluded that coupling is needed to update the surface topography in the RCM to consider all interactions between the near-surface atmosphere and the new morphology."

Becomes:

"The offline correction is no longer valid on the ice sheet margins because **it fails to consider the mitigation of temperature lapse rate** and, consequently, melt due to changes in topography, such as retreat and changing slope. These changes influence the wind regime at the margins of the MAR-coupled simulation. **The mitigation of the melt rate depends on the reduction of sensible heat flux due to changes in local wind regimes and temperature lapse rates. A hypothesis to explain these local changes around the ice sheet margins involves modifications of barrier wind regimes. These winds typically** act along the ice sheet, transporting warmer air from the tundra, enhancing the surface melt, and increasing the northward wind speed along the west side, for instance. The orographic barrier, crucial for the formation of such winds, diminishes with the evolving topography in MAPI-2w. Consequently, enhanced melt **could be** mitigated as well as melt-elevation feedback. However, further investigation is required **to validate this assumption and to understand the underlying physical processes responsible for these local wind regime changes initiated by modified surface topography in a warmer climate. A moment budget comparing both simulations (MAPI-1w and -2w) could help identify differences in local atmospheric circulation patterns leading to such variations in representing melt rates at the margins (van Angelen et al., 2011). Additionally, a complete analysis focusing on characteristics such as recurrence and synoptic situation favourable to the development**

of these wind events will be necessary to gain a better understanding of the physical processes involved. In conclusion, the coupling is essential to update the surface topography in the RCM and to consider all interactions between the near-surface atmosphere and the new morphology."

Following comments 3) and 4), we suggest correcting the abstract as follows:

"Abstract. The Greenland Ice Sheet is a key contributor to sea level rise. By melting, the ice sheet thins, inducing higher surface melt due to lower surface elevations, accelerating the melt coming from global warming. This process is called the melt-elevation feedback that can be considered by using two types of models: atmospheric models, which can represent the surface mass balance, usually using a fixed surface elevation, and the ice sheet models, which represent the surface elevation evolution but do not represent the surface mass balance as well as atmospheric models. A new coupling between the regional climate model MAR (Modèle Atmosphérique Régional) and the ice sheet model PISM (Parallel Ice Sheet Model) is presented here following the CESM2 (SSP5-8.5) scenario until 2100 at the MAR lateral boundaries. The coupling is extended to 2200 with a stabilised climate (+ 7 ◦ C compared to 1961 – 1990) by randomly sampling the last 10 years of CESM2 to force MAR and reaches a sea level rise contribution of 64 cm. The fully coupled simulation is compared to a 1-way experiment where surface topography remains fixed in MAR. However, the surface mass balance is corrected to the melt-elevation feedback when extrapolated on the PISM grid by using surface mass balance vertical gradients as a function of local elevation variations (offline correction). This method is often used to represent the melt-elevation feedback and avoid a coupling expensive in computation time. In the fully-coupled MAR simulation, the ice sheet morphology evolution (changing slope and reducing the orographic barrier) induces changes in local atmospheric circulation. More specifically, wind regimes are modified which influences the melt rate at the ice sheet margins. We highlighted a mitigation of the melt lapse rate on the margins by modifying the surface morphology. The lapse rates considered by the offline correction are no longer valid at the ice sheet margins. If used (1-way simulation), this correction implies an overestimation of the sea level rise contribution of 2.5 %. The mitigation of the melt lapse rate on the margins can only be corrected by using a full coupling between an ice-sheet model and an atmospheric model."

Becomes:

"The Greenland Ice Sheet is a key contributor to sea level rise. By melting, the ice sheet thins, inducing higher surface melt due to lower surface elevations, accelerating the melt coming from global warming. This process is called the melt-elevation feedback and can be considered by using two types of models: atmospheric models, which can represent the surface mass balance (positive degree day, or polar-oriented regional climate models for instance). But these models generally use a fixed surface elevation. And on the other side, the ice sheet models represent the surface elevation evolution. The last ones do not represent the surface mass balance explicitly as well as polar-oriented regional climate models. A new coupling between the regional climate model MAR (Modèle Atmosphérique Régional) and the ice sheet model PISM (Parallel Ice Sheet Model) is presented here following the CESM2 (SSP5-8.5) scenario until 2100 at the MAR lateral boundaries. The coupling is extended to 2200 with a stabilised climate (+ 7 °C} compared to 1961–1990) by randomly sampling the last 10 years of CESM2 to force MAR and reaches a sea level rise contribution of 64 cm. The fully coupled simulation is compared to a 1-way experiment where surface topography remains fixed in MAR. However, the surface mass balance is corrected for the melt-elevation feedback when interpolated on the PISM grid by using surface mass balance vertical gradients as a function of local elevation variations (offline correction). This method is often used to represent the melt-elevation feedback and prevents from a coupling too expensive in computation time. In the fully-coupled MAR simulation, the ice sheet morphology evolution (changing slope and reducing the orographic barrier) induces changes in **local atmospheric patterns**. More specifically, wind regimes are modified, **as well as temperature lapse rates,** which influence the melt rate **through the modification of sensible heat fluxes at the ice sheet margins.** We highlighted mitigation of the melt lapse rate on the margins by modifying the surface morphology. The lapse rates considered by the offline correction are no longer valid at the ice sheet margins. If used (1-way simulation), this correction implies an overestimation of the sea level rise contribution of 2.5 %. The mitigation of the melt lapse rate on the margins can only be corrected by using a full coupling between an ice-sheet model and an atmospheric model."

Minor comments

1. p2 L25 The statement that the offline correction works well as long as SMB is mainly dominated

by elevation could be reformulated. In principle there is no reason to believe that the SMB is

a linear function of elevation, yet this is what is observed in the field.

To clarify this sentence we propose to add that SMB is linked to the surface elevation through the temperature lapse rate.

"Using an offline correction works well as long as SMB (particularly melt) is mainly influenced by the surface elevation."

Will be replaced by:

"Using an offline correction works well as long as SMB (particularly melt) is mainly influenced by the surface elevation **through the temperature lapse rate**."

2. p2 L34 "What becomes GrIS" should be reformulated

"First, the aim is (1) to analyse what becomes GrIS in 2200 with this new coupling following an extreme scenario."

will be replaced by:

"First, the aim is (1) to analyse **the evolution of the GrIS by 2200** with this new coupling following the influence of an extreme scenario."

3. p3 L16 ”good performance”. Providing some numbers would be useful to better describe the uncertainties in modelled SMB by regional climate models. It would also help to mention that the evaluation of MAR by the authors (Delhasse et al, 2020) was using a higher horizontal resolution in MAR (15 km). See also Major comment #2.

In Delhasse et al. (2020) SMB from MAR is not directly compared to SMB-observation, but near-surface climate variables, determinating SMB in MAR are. What we can add here is that MAR forced by reanalyses better perform to represent near-surface temperature that ECMWF-reanalyses themselves compared to in-situ observation. We can also specify that the resolution was finer in the evaluation paper.

4. p6 It would be useful to extend Figure 1 with the initialisation steps to better understand section 2.3.1.

This remark was already formulated by the editor at the first read. We propose so to integrate into Figure 1 the extension realised for the editor's comment:

5. It is not clear why the surface mass balance is sometimes referred to as ”(surface) mass balance” (e.g. p7 L29) or ”surface mass balance”. Using the same would improve readability.

Thanks for this remark, it seems that it was not very clear. We just mean mass balance and surface mass balance. But we will change and clarify with both abbreviations: MB and SMB.

6. p8 I believe there is something wrong with the notation of mass loss in L2 :”-50 Gt.10−3”. Should it be 50 10.3 Gt ? Also in L4.

It’s not a mistake, but we should specify this is the value of total mass balance, which is negative.

7. p1 L5 What is meant exactly by ”as well” ? Do the authors refer to the performance of degree-day models in ice sheet models ? Please be more specific.

To clarify, we propose to change the sentence as follows:

“[…] atmospheric models, which can represent the surface mass balance, usually using a fixed surface elevation, and the ice sheet models, which represent the surface elevation evolution but do not represent the surface mass balance as well as atmospheric models.”

Will be rephrased as:

“[…] atmospheric models, which can represent the surface mass balance **(Positive degree day, or regional climate models for instance)**, usually using a fixed surface elevation, and the ice sheet models, which represent the surface elevation evolution. **These last ones do not represent the surface mass balance explicitly as well as atmospheric models.**”

8. p8 The unit of the y-ax in fig 8b is missing. Adding the variables of each ax would also increase readability.

Thanks for these remarks, we will modify them in consequence.

9. p14 L3. Why is only the north-south wind component investigated, and not the wind vector or even the entire vertical profile of modelled wind speeds ? The latter would give a clear indication of changing boundary layer structure and therefore surface fluxes.

In order to demonstrate and illustrate the changes in barrier winds, we have only proposed the north-south component of the wind, which shows that speeds in this direction are less pronounced at the margin of the ice sheet in the coupled mode in MAR. The east-west component, as well as the mean wind speeds, have of course been analysed, but are not illustrated here because the changes observed do not allow us to justify the differences in terms of temperature, and therefore in terms of melting and SMB, between the results from the coupled and uncoupled simulations. These illustrations can of course be added as supplementary material.

Technical comments

- o1 L10 ”to” − > ”for”
- p1 L12 ”avoid”
- p5 L8: (Franco et al, 2012)
- p5 ”2.3.1 Inisialisation”
- p17 L16 ”Do”

Thanks for all these technical comments, they were all considered in the revised manuscript.

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Hermann M, Box JE, Fausto RS, et al (2018) Application of PROMICE Q-Transect in situ accumulation and ablation measurements (2000-2017) to constrain mass balance at the southern tip of the Greenland ice sheet. J Geophys Res Earth Surf 123:1235–1256. https://doi.org/10.1029/2017JF004408.

We first would like to thank Reviewer#2 for the thoughtful comments and well complete review which will help to improve our manuscript.

The paper by Delhasse et al. presents a coupling methodology between the regional atmospheric model (forced by the large-scale CESM2 climate model) and the PISM ice-sheet model over the 1991-2200 period. Different levels of complexity for the coupling are compared: A real two-way coupling, a one-way coupling in which only the melt-elevation feedback is accounted for through an offline correction and, finally, the no coupling scheme where MAR is run with a fixed topography of the Greenland ice sheet and PISM is forced by the surface mass balance and the surface temperature computed by MAR. A focus is made on the role of the melt-elevation feedback and the barrier winds on the evolution of the Greenland ice sheet geometry (and the consequences on ice surface velocities), the surface mass balance and its components (runoff, meltwater production solid and liquid precipitation) and the contribution of Greenland ice sheet to sea level rise. Most often the results are generally presented in a fairly clear manner, although some parts of the paper require further explanation or analyses (see Specific Comments). This study is similar to the one conducted by Le chlec'h et al. (2019) that was based on the coupling between MAR (forced by the CMIP5 climate model MIROC5) and another ice-sheet model (GRISLI), despite a slightly different methodology to extend the warming scenario beyond 2100. I find it very instructive to compare two modelling studies done with similar approaches but different models. The discussion provides a detailed comparison explaining the different conclusions of these two studies. Overall, I find the study of Delhasse et al. deserves to be published after a number of revisions have been addressed.

Specific Comments

In several places, I think the paper would be of better quality if some clarifications or additional explanations were made.
1/ First of all, I think that a more extensive description of the PISM model is required. The description provided in the paper is too technical, if not incomprehensible,for a reader who is not familiar with ice sheet dynamics models. Although a number of references are given in which PISM is described, I think it is important to be able to understand the important points of this section without going to look for the references mentioned. Also, I think it is necessary to define a number of notions, such as SIA, SSA, the Mohr-Coulomb criterion, exponent of the sliding law, flow enhancement factor, etc... This list is obviously not exhaustive and some equations (and their meaning) would allow a better understanding of how the model works. For example, in figure 6 you represent the driving stresses. But how are these driving stresses calculated? Since you mention in the description of PISM the aspects related to the dynamics of the ice, it would be interesting to refer to them more in the analysis of the results, or else to keep in the description of the model only the characteristics used for the analysis of the simulations.

Yes, we agree and explain now a bit more the PISM setting and try to communicate to a broader audience. However, this section is very technical because it should also inform other ice sheet modelers about the ice sheet settings we used, such that they can reproduce or improve this experiment. Explaining all equations would go beyond the scope of this study and readers should refer to references describing PISM.

We propose to revide the entire section 2.1.2 about PISM description as follows:

“To represent the dynamics and surface elevation of the Greenland Ice Sheet (GrIS), we utilise the Parallel Ice Sheet Model (PISMv1.2.1), a high-resolution numerical ice-sheet/ice-shelf model (Bueler and Brown, 2009; Winkelmann et al., 2011). In PISM, the geometry, temperature, and basal strength of the ice sheet are incorporated into stress balance equations at each time step to determine the ice velocity.

PISM employs two approximations for shallow ice sheets: the Shallow Ice Approximation (SIA) and the Shallow Shelf Approximation (SSA). The SIA is suitable for slowly flowing ice that deforms under its own weight, assuming a strong connection between the ice base and the bedrock. The softness of the ice, affecting its flow velocity, is modulated by an enhancement factor, which we set to E = 3 in our experiments. Faster flowing ice, such as ice streams, glaciers, and shelves, is approximated using the SSA. PISM combines both approximations into a hybrid stress balance mode (Bueler and Brown, 2009; Aschwanden et al., 2012).

Basal sliding of the ice over the bedrock introduces basal resistance. The speed of basal sliding is determined by the sliding law, typically a power law relating to the basal shear stress and yield stress. In our study, we adopt the Mohr-Coulomb criterion and use an exponent of q = 0.6 for the sliding law.

The model considers basal resistance based on the hypothesis that the ice sheet rests on a till layer. The yield stress represents the strength of this aggregate material at the base of an ice sheet. When yield stress is lower than the driving stress (τd) there is likely to be sliding, and thus faster velocities can be observed. The driving stress in turn is dependent on the ice thickness (H) and surface gradients (Hs) of the ice: τd ∝ Hs. The thicker and steeper the ice, the higher the driving stress and most probably the ice velocity.

The properties of the till are further approximated by using material properties such as the friction angle. We vary the till friction angle linearly between 5° and 40° with respect to bedrock elevation (between -700m and 700m), following Aschwanden et al. (2016). This variation in friction angle leads to lower friction at lower altitudes and below sea level, resulting in increased surface velocities at the margins of the ice sheet, thus improving the match of flow structure for the glaciers.

To match the present-day extent of the ice sheet, we impose a strong negative surface mass balance (SMB) at the margins of the Greenland present-day ice mask. This setup allows only for ice retreat in our experiments. We also enforce a minimum thickness of 50 m for floating ice at the calving front and utilize the von Mises calving law, which is suitable for glaciers in Greenland (Morlighem et al., 2016). All other parameters are set to default values (University of Alaska Fairbanks, 2019). Our simulations do not consider bedrock deformation or changes in ice-ocean interaction, as we maintain constant submarine melt rates.”

2/ The GRIP record does not extend to 125 ka as the signal was perturbed at the bottom of the ice core. While, I think that it shouldn't affect much your results, this should be mentioned. Also, the original publication should be provided (Johnsen et al, 1992) instead of Johnson et al. (2019).

Yes, we cite the dataset here, as it includes several data sources. For clarity, we write now (P4, L24): "The historical time series (Johnson et al., 2019) ***includes the temperature*** derived from Oxygen Isotope Records from the Greenland Ice Core 25 Project (GRIP, Johnson et al., 1992)"

3/ The initialisation method of PISM and the coupled model could be better explained with a scheme. For example, you mention that during the first step of your initialisation PISM is forced with the 2D temperature anomalies of the last glacial cycle. Are these temperatures correspond to the internal temperatures? If so, these anomalies are not a forcing but an initial state for the next step of the initialisation. Other clarifications are needed: Explain why do you need to equilibrate the vertical temperature profile. This is not clear in the manuscript. How these anomalies are computed? What is the impact of using anomalies instead of absolute values?

For a glacial spinup it is assumed that the state of the ice sheet before a glacial cycle is equal to the one at present day, which means that ice topography and surface temperatures are as well. However, different surface topographies lead to different surface temperatures (which we achieve during our coupled spinup runs.) This is why it is common practice to use temperature anomalies over the last glacial cycle, because the assumption of equal glacial states before and after the glacial cycle only holds when using anomalies. The temperature anomalies are transferred to the surface, which gradually modifies the ice interior temperatures over time. The process of surface temperatures penetrating down into the ice column takes several thousand years due to the thickness of the ice, which can be several kilometers. The internal temperature profile of the ice in turn, determines its softness and deformability, thus affecting the flow velocity of the ice.

To summarize, in our glacial spinup process, we initialize the ice sheet model with temperature anomalies to account for different surface topographies. These anomalies gradually modify the ice interior temperatures over time, influencing the flow behavior of the ice.

We propose to complete section 2.1.3 as follows (since P4, L. 19):

"PISM is forced by yearly ST and SMB from MAR forced by CESM2. To achieve a stable spinup state, we forced PISM with the MAR mean fields (ST and SMB) over 1961 – 1990, when the GrIS was close to balance (Fettweis et al., 2017). However, for a realistic thermodynamics representation of the ice sheet, the temperature evolution of the last glacial cycle has to be considered"

Becomes:

“PISM is forced by yearly ST and SMB from MAR forced by CESM2. To achieve a stable spinup state, we forced PISM with the MAR mean fields (ST and SMB) over 1961 – 1990, when the GrIS was close to balance (Fettweis et al., 2017). However, for a realistic thermodynamics representation of the ice sheet, the temperature evolution of the last glacial cycle has to be considered, ***because the surface temperature slowly propagates down the ice column and determines the vertical ice profile of the ice sheet. The ice profile determines the ice softness and deformability, thus affecting the flow velocity of the ice.***

For a glacial spinup, it is assumed that the state of the ice sheet before a glacial cycle is equal to the one at present day, which means that ice topography and surface temperatures are as well. However, different surface topographies lead to different surface temperatures (which we achieve during our coupled spinup runs.) This is why it is common practice to use temperature anomalies over the last glacial cycle, because the assumption of equal glacial states before and after the glacial cycle only holds when using anomalies.”

We also propose to add Figure R1 in Section 2.3.1 (Initialisation of the coupling) to illustrate both the PISM spinup and the coupling spinup:

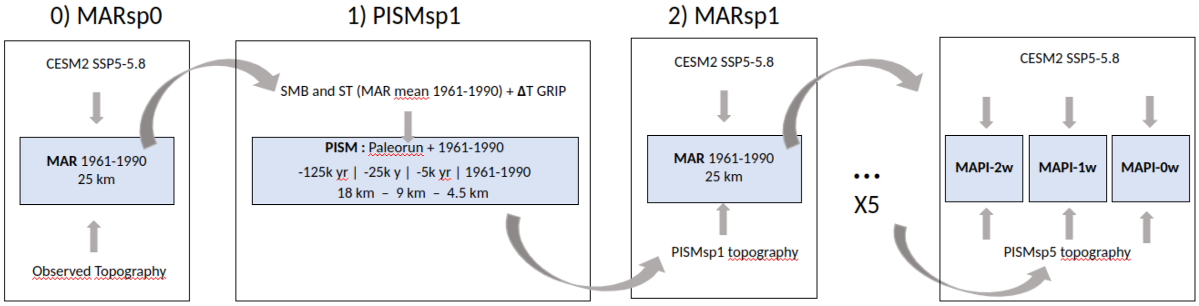

Figure R1. Steps of the coupling initialisation. Each MAR step corresponds to a 30-year long run over the reference period (1961-1990). And each PISM step consists of a new initialisation cycle of PISM as described in Section 2.1.3.

4/ a) The offline correction method is a key component of the overall paper as is drives the melt-elevation feedback, but the description of the method is very short. Although, I think I understand the basic principles of the method, I found that more details would have been welcome, and possibly a scheme to better illustrate the method.

b) Similarly, I think that the analysis of the results and the comment of Figure 8 are unclear (as well as the conclusions drawn from this figure) are unclear. For example, the authors mention "The dependence is no longer linear" (P12, L20). This just means that the temperature gradients are not the same in the two experiments, if I understood correctly. In other words, the altitude correction is not the same in both experiments. Again, this part of the analysis would be better understood if the authors had given more details about the correction method.

c) Also, I am not sure I agree with the first conclusion of this analysis, namely, "the linear correction is no longer valid in the ice sheet margins". I don't think the analysis leads to this conclusion if the altitude correction is different in the two experiments. Perhaps it would have been better to plot the regressions for each experiment independently (not as anomalies) and to examine the slopes of the regression lines separately.

d) Also, at the ice sheet margins, the behavior is not the same for altitude differences below ~350 m and above. This could be mentioned.

With this comment, we well realised that the description of this offline correction really needs to be revised. We answer these comments in 4 points (a to d) to be clear:

4.a) As asked by the first reviewer too, we propose to revise the description of the offline correction and add an illustration, to better illustrate what's done.

Here are corrections in the text including two more figures (this is the same answer as in review#1, first major comment):

P5. L. 3-10: "Before any data exchange between the models, data has to be interpolated on the destination grid because the two models were run at two different spatial resolutions (25 vs 4.5 km). The surface elevation simulated by PISM is then interpolated using a four-nearest-neighbour distance-weighted method on the MAR grid at 25 km. For the MAR variables, they are interpolated using the same method on the PISM grid at 4.5 km. However, they are further corrected by considering the difference in altitude between the two grids at the time of interpolation thanks to local vertical SMB/ST gradients. This method is described in (Franco et al., 2012) and is called offline correction hereafter. This method corrects the altitude-dependent variables (such as SMB and ST) by applying a local linear gradient of the variable according to the surface elevation differences between the current MAR grid cell, and the surrounding MAR grid cells (9 grid cells considered here to compute the vertical gradient)."

Become:

"Before any data exchange between the models, data has to be interpolated on the destination grid because the two models were run at two different spatial resolutions (25 vs 4.5 km). The surface elevation simulated by PISM is then interpolated using a four-nearest-neighbour distance-weighted method on the MAR grid at 25 km. The MAR variables are interpolated using the same method on the PISM grid at 4.5 km. However, they are further corrected by considering the difference in altitude between the two grids at the time of interpolation thanks to local vertical SMB/ST gradients. This method is described in (Franco et al., 2012) and is called offline correction hereafter. Firstly, a linear and elevation-dependent gradient (Figure R2) is calculated over the MAR grid by considering the values of the considered variable (SMB at 4.5 km in our example, Figure 1) of the eight surrounding grid cells of the current one. This gradient is specific to each PISM-grid-cell and is locally determined. An example of this gradient can be found in Figure RS1 in The Supplement. Subsequently, These gradients are used to correct the variable when it is interpolated onto the PISM grid. The correction is performed by multiplying the interpolated variable by the difference in surface elevation between the grid cells in MAR and in PISM. This offline correction is specifically employed to correct variables that are influenced by temperature lapse rate with altitude, namely temperature and derived variables.”

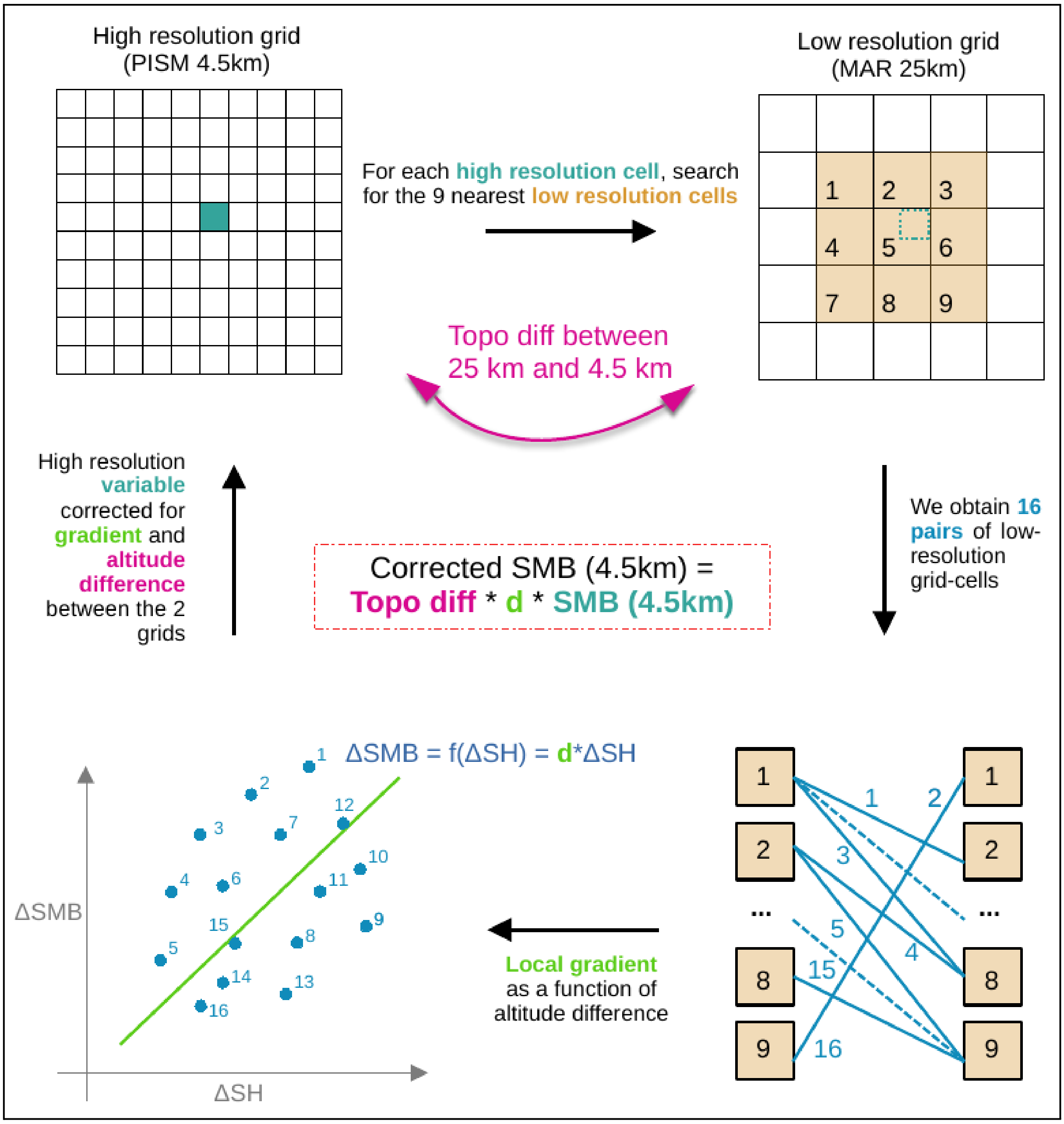

Figure R2. Steps of the offline correction as described in Franco et al. (2013). After interpolation of a variable (SMB, surface mass balance, in this figure) from a low to higher resolution grid, this variable is corrected to consider the influence of the temperature lapse rate with altitude. The correction is based on a local gradient (d) calculated by considering SMB differences (ΔSMB) between 9 nearest grid cells in the neighbourhood of the current one in the source grid in function of the surface elevation difference (ΔSH). Modified from Wyard (2015).

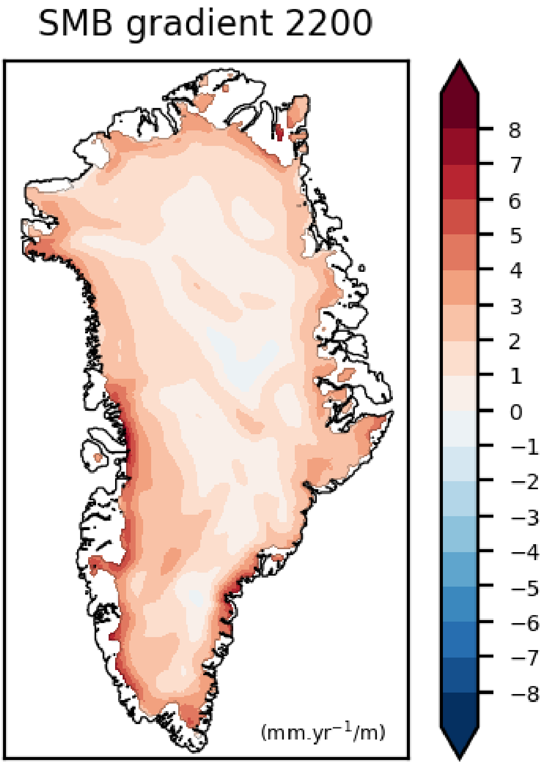

Figure RS1. Surface mass balance (SMB) gradients used to correct SMB as modelled by MAR (25 km) when interpolated on PISM grid (4.5 km) in 2200 by the MAPI-1w run (MAR-PISM uncoupled). Gradients (mm.yr-1/m) are multiplied by the difference in surface elevation to correct the rough SMB.

4.b) Similarly, I think that the analysis of the results and the comment of Figure 8 are unclear (as well as the conclusions drawn from this figure) are unclear. For example, the authors mention "The dependence is no longer linear" (P12, L20). This just means that the temperature gradients are not the same in the two experiments, if I understood correctly. In other words, the altitude correction is not the same in both experiments. Again, this part of the analysis would be better understood if the authors had given more details about the correction method.

Figure 8 highlights the spatial modification of the dependence between altitude and temperature, and consequently between altitude and melt. This is possible because we can compare a simulation where the topography has remained fixed (MAPI-1W) with an identical simulation where only the topography changes (MAPI-2w). So here we compare the results on the MAR grid of these two simulations, before applying the offline correction, each year at the same place. We are not comparing the different gradients used by the correction. Before running the simulations, we expected to find similar dependencies between the evolution over time of topography and temperature over the entire ice sheet. Thus the main expectation was that the gradients found here, by comparing the results over time, would give gradients similar to those calculated by the correction spatially.

Figure 8a.) highlights that in the center of the ice sheet (in blue), when the topography is modified in MAR, melt and temperature evolve linearly with these changes in topography, compared with the results obtained when the altitude remains fixed. In the center we can therefore say that 99% of the temperature changes (94% for melt, figure 8b.) can be explained by the change in surface elevation ($R^2 = 0.99$ and 0.94 rsp.), with a lapse rate of ~ -0.4°C/100m (regression slope) when we compare the annual temperatures obtained with a fixed topography and with a variable topography over time.

However, when we compare the same results for temperature and melt on the inner margins of the ice sheet (relations in green in Figure 8), we notice that only 61% ($R^2 = 0.61$) of the changes in temperature are explained by changes in surface elevation (69% for melt). We all agree that the term non-linearity is perhaps a little strong. But the key message here is that the relationship no longer reflects such a strong dependency (R^2 are lower). This proves that other factor(s) explain(s) these temperature changes. We have interpreted this as a factor that mitigates the dependence of temperature on altitude, and therefore the melt-elevation feedback.

The point about the offline correction is that it is based on the theory that changes in surface elevation lead to changes in temperature following temperature lapse rates, and this has a direct influence on melt, SMB and other temperature-dependent variables.

To avoid any confusion in Figure 8 with the correction, we propose to add, in the main text, values of the correction gradients used at these specific locations for both temperature and melt (Table R1). Note firstly that these gradients, for the interior of the ice sheet, are relatively close to those calculated in our example (regression slopes). Secondly, at the margin of the ice sheet, the gradient used in the correction is similar to that inside the ice sheet. However, thanks to our comparison in Figure 8, we know that by modifying the topography, the temperature and melt gradients in the margins are mitigated. It is therefore clear that the correction applied to the simulation with a fixed topography will give higher melt rates than expected at the edges of the ice sheet to the simulation with varying topography where all the processes and feedback resulting from elevation changes are considered. The correction we apply therefore does not take into account the process(es) that attenuate the melt-elevation feedback. This/these process(es) mitigate, as a consequence, the dependence relationship between the temperature and altitude by around 10 to 20%.

The gradients computed for the offline interpolatio from the fixed MAR grid towards PISM grid (MAPI-1w) in the same specific locations as Figure 8 are given in Table R1. Given a 200m difference in surface elevation between the fixed MAR grid and the PISM grid in 2200, this will lead to a correction of 990mm (-4.95 mm x -200m) inside the ice sheet. Similarly, we obtain a correction of 1052.5mm (-4.2 mm x -250m) at the margins. However, following our comparison over time of MAR coupled and running with fixed topography (Figure 8), the gradient in the margin should be much lesser important compared to inside the ice sheet; -4.95 inside VS -4.21 mm/m at the margins as calculated by the correction, and -3.30 VS -1.34 mm/m by comparing the two simulations. So as the gradient does not consider mitigation of the temperature lapse rates, nor the consequent mitigation of the melt-elevation feedback, the melt correction applied is too important and artificially increases the melt rate. The correction in the margins is too strong and results in underestimating the SMB in these areas.

Calculated on MAPI-1W	Temperature gradients (°C/m)	Melt gradients (mm/m)
INSIDE (49.24 °W, 67.07°N)	-0.0069	-4.95
MARGIN (48.83°W, 67.08°N)	-0.0065	-4.21

Table R1. Temperature and mel local gradients as considered by the offline correction in 2200 for the MAPI-1w simulation at two locations, one inside and one on the margin of the ice sheet. These locations are the same as in Figure 8 of the manuscript.

To better explain this figure in the main text, we propose to adapt as following:

P12, L14 - P13, L5:

“The underestimation of SMB in MAPI-1w is due to an overestimation of the melt-elevation feedback by the offline correction when interpolating MAPI-1w towards the PISM grid compared to the explicit consideration of this feedback in MAPI-2w. This correction is based on the linear temperature dependence with the surface elevation to account for the melt-elevation feedback that alters the SMB and related variables. The correction applies local linear gradients according to these altitude differences. We compare, on the MAR grid, the yearly evolution of the altitude differences between the two experiments (coupled and uncoupled) with the evolution of the temperature differences inside the ice sheet and on the margin (Fig. 8a). We notice that on the margin, the dependence is no longer linear (analyse for different other grid cells have been carried out but are not shown here). Inside the ice sheet, the temperature-elevation relation, evaluated as -0.4 °C/100m, remains linear. In our example, modifications of the topography in the 2-way coupling experiment have modified this linear relationship to -0.1 °C/100m of the temperature increase with the surface elevation lowering along the margins. The same relationship is illustrated for melt differences (Fig. 8b), confirming the modification in linear dependence with changes in surface elevation. This highlights two main elements: (1) the linear-offline correction of SMB is no longer valid in the ice sheet margins; (2) the non-linear relationship between temperature and altitude driving the melt-elevation feedback leads to mitigation of this feedback along the ice sheet margins.”

Become:

“The underestimation of SMB in MAPI-1w is due to an overestimation of the melt-elevation feedback by the offline correction when interpolating MAPI-1w towards the PISM grid compared to the explicit consideration of this feedback in MAPI-2w. This correction is based on the linear temperature dependence with the surface elevation to account for the melt-elevation feedback that alters the SMB and related variables. The correction applies local linear gradients according to these altitude differences. We compare, on the MAR grid, the yearly evolution of the altitude differences between the two experiments (coupled and uncoupled) with the evolution of the temperature differences inside the ice sheet and on the margin (Fig. 8a). **We notice that on the margin, differences in altitude between the two MAR-grid (ΔSH) explain only 61% (69% for melt) of the changes in temperature differences (ΔT2m and ΔME respectively), compared with the interior of the ice-sheet where these relationships are much more dependent, with R^2 of 0.99 and 0.94 respectively.** In our example **(Fig. 8a),** modifications of the topography in the 2-way coupling experiment have modified this linear relationship **with the temperature from -0.4 °C/100m inside** to -0.1 °C/100m. The same relationships are illustrated for melt differences (Fig. 8b), confirming the modification in linear dependence with changes in surface elevation. **We will now compare these gradients, obtained by comparing the MAR simulations with and without changes to the topography over time, with the gradients used by the offline correction. These are calculated locally, i.e. taking into account the differences in altitude and in the variable considered with the surrounding grid cells. For the example of the temperature, we find gradients of -0.69 and -0.65 °C/100m in 2200 respectively for the same locations as in Fig. 8 inside the ice sheet and on the margins. Although in absolute values these gradients are different from those obtained by comparing the two simulations over time, the difference between the two regions is smaller. The gradient applied to the margin of the ice sheet follows a similar dependency to that of the interior of the ice sheet. This explains the exaggeration of temperature and temperature-dependent variables (melt, SMB, etc.) on the margins by the correction, given the use of a gradient that is too large and does not reflect processes leading to the mitigation of the temperature altitude dependence, and consequently, melt-elevation feedback. All these comparisons** highlight two main elements: (1) the linear-offline correction of SMB is no longer valid in the ice sheet margins; (2) **the changes in the linear** relationship between temperature and altitude driving the melt-elevation feedback lead to mitigation of this feedback along the ice sheet margins.”

4.c) Also, I am not sure I agree with the first conclusion of this analysis, namely, "the linear correction is no longer valid in the ice sheet margins". I don't think the analysis leads to this conclusion if the altitude correction is different in the two experiments. Perhaps it would have been better to plot the regressions for each experiment independently (not as anomalies) and to examine the slopes of the regression lines separately.

Figure 8 does not present the corrections applied in the two experiments, but the dependencies that exist over time between changes in altitude and temperature, and between altitude and melt, as explained in comment b) above. However, as detailed above, we propose to extend our comment on this figure and add a comparison with actual gradients used by the offline correction.

4.d) Also, at the ice sheet margins, the behavior is not the same for altitude differences below ~350 m and above. This could be mentioned.

That's right, beyond a difference of 300-350m it seems that the relationship is modified. We can point this out in our results. Without reaching values like those inside the ice sheet, it seems that beyond these differences in altitude, the relationship seems to get closer to the expected one. This could perhaps mean that we are dealing with something temporary, or that after a certain drop in altitude, the well-known dependence between altitude and temperature takes over again in terms of influence. This would have to be verified at different locations in the zones affected by mitigation of melt elevation feedback before any conclusions could be drawn.

5/ The role of barrier winds seems to be a key element to explain the differences in the melt-elevation feedback between MAPI-2W and MAPI-1W. Could it be mentioned more explicitly in the abstract?

As explained in the 1st review, mitigation of barrier winds is one plausible hypothesis to explain why we have here a mitigation of the melt elevation feedback by directly using an evolving topography in MAR. To confirm this hypothesis, it should be better to process a complete wind budget of the two simulations and highlight differences. As it's not really the first aim of this paper, we prefer to keep this as a hypothesis. This is why we prefer to not emphasise this explanation directly in the abstract.

6/ Overall, the paper is written in understandable English. However, I think that the quality of the manuscript would be greatly improved if it were proofread by a native English speaker.

We will make sure to improve the English written expression of the manuscript when we revise it.

Minor comments

P1, L3: While MAR is able to diagnose the ice sheet surface mass balance thanks to the implementation of snow/ice layers, this is not the case for many atmospheric models. The statement "atmospheric models which can represent the SMB" should be tempered.

Thanks for your comment, we propose to adjust the sentence as follows:

"This process is called the melt-elevation feedback that can be considered by using two types of models: atmospheric models, which can represent the surface mass balance, usually using a fixed surface elevation, and the ice sheet models, which represent the surface elevation evolution but do not represent the surface mass balance as well as atmospheric models. "
Become

"This process is called the melt-elevation feedback that can be considered by using two types of models: atmospheric models, which can represent the surface mass balance **for some of them, especially polar-oriented regional climate models. But they usually use a fixed surface elevation. And the other side,** ice sheet models which represent the surface elevation evolution but do not represent the surface mass balance as well as atmospheric models. "

P2, L31: “As the coupling is dependent on the used ISM à What do you mean? I guess that you mean that the results of your coupled simulations are model-dependent? Please, clarify.

We specify here that the coupling is model-dependent, specifically concerning the ISM-used, due to the divergence of the results in similar conditions of different models over Greenland (ISMIP Greenland 5 & 6). A coupling is less dependent of the RCM used, as SMB resulting from different RCM are quite similar over the recent period over Greenland. By saying that the coupling is dependent on the used ISM, we refer to and sum up the explanation given in P2, L14-17.

Section 2.13: It seems to be that the abbreviation ST has not been defined before. Also, explain why PISM needs to be forced with ST.

We now explain why we need a forcing of surface temperature. (see answer to major comments above. We introduce “ST” in section 2.1.1.

P4, L-25: The GRIP record was perturbed at the bottom and did not extent to -125 ka. This should affect your results so much but should be mentioned.

Yes, the dataset includes GRIP data but also had other sources so that we could simulate temperatures until -125 ka. See answers to major comments.

P6: L13: MARref forced with CESM2 is also run with PISMsp5 topography (see previous sentence). This sentence is a bit confusing. Please rephrase. Also change PSIMsp5 in PISMsp5.

Yes exactly, both MAR simulations (with CESM2 and ERA5) are running with the same topography from PISMsp5. To be clear, we propose to adjust our sentences as follows:

“The PISMsp5 topography, the last step of the initialisation process, will be the initial state of the different simulations compared here and is used to run the MAR reference simulation over the reference period (MARref). As our projections could not be evaluated, we compared performances of MARref forced with CESM2 to MAR using the PSIMsp5 topography and forced with the observed climate, i.e. the reanalysis ERA5 here (Hersbach et al., 2020). “

Become:

“The PISMsp5 topography, the last step of the initialisation process, will be the initial state of the different simulations compared here and is used to run the MAR reference simulation over the reference period (MARref). As our projections could not be evaluated, **we evaluated the performances of MARref over the present. To do so, we compared MAR results over the current period (1961-1990), with the initialised topography (PISMsp5) forced on one hand, by the ESM used for projections (CESM2) and on the other hand ERA5 reanalysis (Hersbach et al., 2020), considered as observations and well representing current climate.**”

At different places in the manuscript there is confusion between interpolation and aggregation. Outputs coming from a higher resolution model are *aggregated* on a coarser model grid. Variables computed with a lower resolution model are *interpolated* on a finer model grid.

Yes true, we didn't make the distinction, but we will correct that, thanks.

Section 2.4: You should add a comment explaining why you deal sometimes with surface mass balance and sometimes with mass balance. Also, explain (maybe before section 2.4) the difference between both.

The surface mass balance (SMB) is one of the components of the total mass balance of the ice sheet, call here mass balance (MB). The SMB is obtained by using the MAR model and summarises the gains and losses of ice mass at the surface, whereas the total mass balance is the result of computing both SMB and the dynamic of the ice sheet. MB is then the PISM result. In our main text, depending on which component we refer to, we talk about SMB or MB.

To address your request, we propose to specify again which model computes which part of the total mass balance in section 2.2, when we describe the coupling.

P4. L30-31 and P5. L1-2: "The coupling between both models has been performed by exchanging yearly outputs (SMB and ST from MAR, and ice thickness from PISM) on the 1st January of each year for 1991 – 2200 as described in Le clec'h et al. (2019). For MAR, this induces updating the surface elevation and ice extent of the ice sheet at the beginning of each year with PISM results from the previous year, whereas SMB and ST are used as forcing fields for PISM."

"The coupling between both models has been performed by exchanging yearly outputs (SMB and ST from MAR, and ice thickness from PISM) on the 1st January of each year for 1991 – 2200 as described in Le clec'h et al. (2019). For MAR, this induces updating the surface elevation and ice extent of the ice sheet at the beginning of each year with PISM results from the previous year, whereas SMB and ST are used as forcing fields for PISM. **The coupling aims to produce estimations of total MB of the GrIS by simulating dynamical components directly with PISM and using the SMB component as simulated by MAR as forcing for PISM.**"

P8, L4-L5: The sentence is a bit confusing as the climate is not stabilized just before 2100.

We propose to rephrase and complete this part as follows:

"Since there is an acceleration of the mass loss just before 2100, even with a stabilised climate, mass loss is not stabilised in 2200."

Become:

“Since there is an acceleration of **the warming and consequently of the mass loss before 2100, even by stabilising the climate and then the warming after 2100, the mass loss is still increasing until 2200**.”

P9, L10: “synoptic features of the large-scale CESM2 forcing” à Could it be illustrated with a figure (in the Supplement part for example)?

Yes, it’s a good suggestion. To illustrate the large-scale pattern coming from CESM2 and visible precipitation changes at the end of the 22nd century, we plot these changes with raw CESM2 data (Figure R3). This illustrates the decrease in precipitation on the east coast of Greenland. We also identify an increase in precipitation in the western part. This highlight is added in our result comments in the main manuscript because, mixed with changes in surface elevation explanation, it can explain changes in precipitation observed by 2200 in our coupled results. The text is adapted as follows:

“There is a significant decrease in total precipitation (SF + RF, Fig. 7c) over the southeast due to synoptic features of the large-scale forcing (CESM2, not shown). Conversely, our simulation projects significantly increase over the west and north of Greenland. The increase in the west is a consequence of the ice sheet thinning as clouds can penetrate more inland due to a weaker topographic barrier effect and a delayed condensation due to further lift-up of air masses. **A synoptic pattern coming from the CESM2 forcing is also contributing to this increase in precipitation (not shown).** We attribute changes in snowfall for the north of Greenland to more humidity content associated with atmospheric warming, as this region is particularly dry and cold over present-day conditions.”

Unfortunately, as these CESM2-data are incomplete (only available over persistent iced areas) and we do not have access to complete one (CESM2 simulation used as forcing files from MAR in this study), we prefer to not add this in our Supplement.

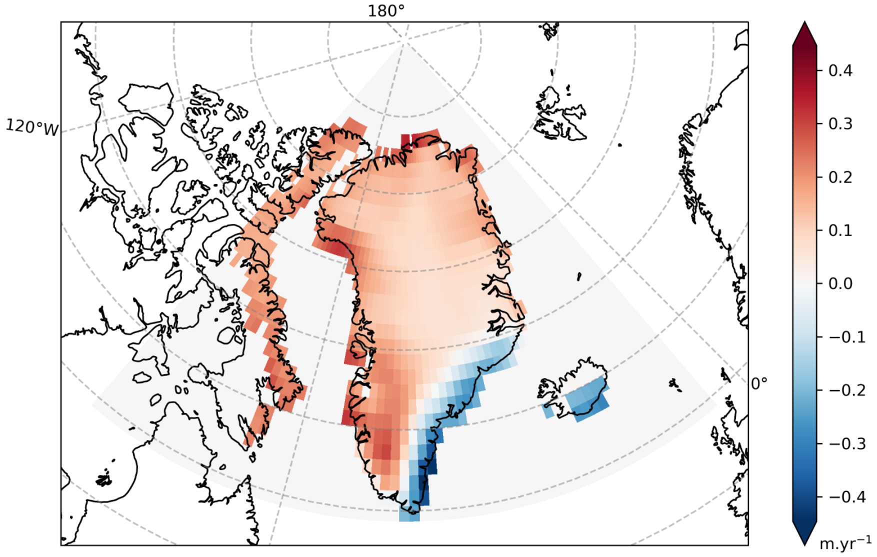

Figure R3. Precipitation (snowfall + rain, mm.yr-1) as simulated by CESM2 over 2071–2100 over the iced surfaces of Greenland region.

P12, L1-2: This sentence is not clear. Je ne comprends pas pourquoi les résultats de MAPI-1W sont contraires à ceux de MAR. I think that the sentence should be rephrased.

As it seems this sentence is confusing, we propose to extend the explanation as follows:

“The MB overestimation by MAPI-1w is contrary to the result of the MAR outputs, where MAPI-2w gave higher melt rates than MAPI-1w (Fig. 4 solid vs dashed lines).”

Becomes:

“The MB overestimation by MAPI-1w could be contrary to the intermediate results from MAR of both MAPI-1w and -2w simulations. **If we look at these results (raw MAR outputs)before interpolation and forcing of PISM, fully-coupled melt rate outputs are higher than in the one-way coupled simulation (Fig. 4 dashed lines). After interpolation, meaning the MAR results interpolated on PISM-grid and which actually forced PISM (Fig. 4 solid lines), MAPI-1w gave higher melt rates than MAPI-2w.**”

P12,L2: Figure 1a indicates that the melt-elevation feedback is taken into account via an offline correction. This seems to be in contradiction with the text.

To be consistent, when we interpolated MAR outputs on the PISM grid in both 1-way and 2-way simulations, we applied the offline correction. The difference is that in the 2-way coupled experiment, as the topography is updated each year in MAR, corrections during this interpolation are negligible compared to corrections applied in 1-way simulation as differences in surface elevation are very short and only due to spatial resolution. The melt-elevation feedback in the full coupling is actually well considered by directly modifying surface elevation in MAR as all SMB components are calculated in changing surface elevation.

P12, L17: To which altitude difference do you refer. This is not clear and justifies a more in-depth explanation of the offline correction method.

We refer here to the difference in altitude between the two grids, MAR and PISM. Concerning the in-deep explanation of the offline correction method, we refer to the answer to the major comment 4.

P13,L6: Remind the link between wind, T2m and SHF, as it appears to be a key mechanism to explain the difference in the melt-elevation feedback in your simulations.

We propose to better explain it as follows:

P13, L6 - P14, L2: “The mitigation of the melt-elevation feedback in the MAR-coupled simulation is explained by the modification of the local atmospheric circulation on the margins around the GrIS. The evolution of the topography in the coupled simulation (for instance, Fig. 9e) causes a decrease in the melt increase with the elevation lowering. As meltwater production depends on the near-surface temperature and the wind through the sensible heat flux, we compare the vertical temperature and wind speed patterns above both simulation topographies along a transect crossing the ice sheet. The example illustrated in Fig. 9 highlights [...]”

Becomes:

“The mitigation of the melt-elevation feedback in the MAR-coupled simulation is explained by the modification of the local atmospheric circulation on the margins around the GrIS. The evolution of the topography in the coupled simulation (for instance, Fig. 9e) causes a decrease in the melt increase with the elevation lowering. **The production of meltwater is the result of a positive energy balance at the surface. More specifically, changes in sensible heat flux (SHF) account for this which is directly proportional to surface temperature and wind speed. We investigate here differences in these two parameters between the two experiments. They are both directly influenced by changes in surface topography between MAPI-2w and -1w (Fig. 11b and d). The near-surface temperature, as well as the north-south wind component, are altered along the margin, specifically the west part of the GrIS in the fully-coupled simulation (general wind speed, as well as west-east wind component differences, are presented in the Supplement, Fig. S7). To better illustrate that, we compare the vertical temperature and wind speed patterns above both simulation topographies along a transect crossing the ice sheet.** The example illustrated in Fig. 11 highlights [...]”

P13, L6-7: This should be illustrated with a figure.

Actually, this sentence was supposed to summarise the entire following paragraph, and the figure chosen to illustrate all of this is the described one all along this paragraph: Figure 9.

P13, L7-8: Fig9e represents a cross section of the topography and not an evolution of the topography. Please, rephrase. The mitigation of the melt-elevation feedback with elevation lowering is better illustrated with Figs 8a and S4a.

The evolution of surface topography is well illustrated with these cross sections as the fixed topography of MAPI-1w corresponds to the starting topography of the 2way simulation. As we also plot the cross-section of the fully coupled simulation topography in 2200, we consider that we illustrate these changes in surface elevation. Of course, this is also illustrated by Figure 3, with changes all over GrIS, and Fig S4a, which illustrates the differences in surface elevation between the two PISM simulations (from MAPI-1w and -2w). However, in Fig. S4a, differences could be due to, on one hand, the differences in SMB apply to PISM, or, on the other hand, to the model divergence itself. As it could be confusing, we won’t add this reference at this place. Furthermore, the goal to illustrate surface changes with the cross-section is to have an idea of the shape modifications too, because the slope is very important when you study wind components.

P14, L4: I don’t understand what you mean by “inside the ice sheet” in parenthesis

We just specify here that the grid-cell is in the margin, but still considered inside the ice sheet, so modifications of fluxes and temperature will be important to consider changes in melt.

P17, L16: “Do not consider” à Please rephrase. I suggest something like “We must not consider” or another equivalent formulation.

The sentence would mean that if we miss the consideration of this feedback, this will result in an underestimation of SLR.

We propose to rephrase as follows:

“Missing this feedback will result in underestimating the projected sea level rise contribution of 10.5%”

Typo and technical comments:

Sections 2.1.3 and 2.3.1: Inisialisation à Initialisation

P1, L5: Remove “as well as atmospheric models”

P1, L10: corrected to à corrected for

P1, L11: extrapolated à interpolated

P1, L12: avoid à avoids or “prevents from a too expensive coupling”

P2, L26: Remove “as forcing”

P2, L35 “assess the offline method” should be changed in “assess the ability of the offline method…”

P3, L1 “feedback” à feedbacks

P3, L1: Replace “as well as “by “ “and which”

P4, L26-27: Provide the resolutions in km, not in meters (as in the other parts of the manuscript).

P5, L5: “For the MAR variables, they are interpolated” à “The MAR variables are interpolated…”

P6: L13: change PSIMsp5 in PISMsp5.

At different places in the manuscript there is confusion between interpolation and aggregation. Outputs coming from a higher resolution model are *aggregated* on a coarser model grid. Variables computed with a lower resolution model are *interpolated* on a finer model grid.

P7, L23: I feel that 10% is not so negligible.

P7, L29: Components refer to SMB. Replace “their components” with “its components”

P8, L4: Replace -200 Gt.10_{-3} by -200 10_{-3} Gt (same thing for L2)

P8, L12: Remove meltwater.

P11, L7: underestimated à underestimates

P12, L3 : become à becomes

P14, L3: at the ice sheet margins / on the ice sheet margins

P18, L14: sensibility à sensitivity

P18, L19: oppositely à in opposite ways ?

Figures

Figure 1a : See comment, P12, L2

Fig. 2: The grey band is not visible. Maybe you could choose a darker colour.

Fig 3: Green and red contours cannot be easily distinguished

Fig. 4 caption: (RU, in green) à (RU, in orange). Precise in the figure caption that solid lines correspond to the coupled experiment

Fig.9: Indicate what do the y-axes and x-axes represent (Figs.9a, 9c, 9e). The line thickness of the black lines in Figs.9b and 9d could be slightly increased.

Thanks for all these technical comments, they were all considered in the revised manuscript.

References

Wyard, C: Évaluation de la pertinence du couplage MAR-GRISLI sur le Groenland. Mémoire de master en sciences géographiques, orientation climatologie, à finalité approfondie, Liège, Université de Liège, inédit, 96 p. 2015.